

the magnetic equator and describing a circular orbit concentric with the earth. The magnetic force due to the earth's magnetic field is directed towards the north, and the deflecting force must be directed towards the centre to keep the corpuscle in its orbit. Applying the well-known rule for electromagnetic deflection, we find that the corpuscle, if negative, must move from west to east.

The question regarding the simultaneity of the occurrence of the positive equatorial storms is a very important one for their physical explanation, for if it takes a time of several minutes for the pulse to travel round the earth, we must suppose that the currents producing the effects are near the earth compared with its diameter, while simultaneity of beginning would indicate very distant systems. The question of simultaneity can only have a definite meaning in the case of the abruptly beginning storms, e.g. the positive equatorial storms ("S" storms), and perhaps the cyclo-median storms. The polar storms, on the other hand, usually set in gradually, and near the auroral zone, where they are strongest, they are of a very local character; sudden changes at one station may have no corresponding sudden change at another; but in the case of these polar storms (cf. Birkeland's work) it is often found that the centres of disturbance fields move slowly, usually along the auroral zone.

It has usually been assumed that the positive equatorial storms set in simultaneously all round the world. The question is very carefully examined in the work of Birkeland, referred to above, for the storm of January 26, 1903. Looking at his figures, we notice that corresponding serrations show small differences in time at different stations, amounting to two or three minutes; but these differences are equally great for neighbouring stations as for more distant ones. The differences for neighbouring stations, which must be due to some error, are not so much caused by faults in the measurements on the time axis and the identification of corresponding points on the curves; they are rather to be considered as faults sticking to the magnetogram itself, for if we take out the time of several points of the disturbance, the time differences for corresponding points for two stations come out nearly constant.

Dr. R. L. Faris and Dr. Bauer, who have made a great amount of valuable work on the subject, have tried to eliminate the error by collecting neighbouring stations into groups, and then taking the difference between the average time of each group, and they arrive at the conclusion that the occurrence is not simultaneous. But so long as the differences between the groups are of the same order as the actual possible error of determination, it seems very dangerous to conclude to a non-simultaneity. Moreover, Mr. Krogness, by comparing the times of beginning of a number of storms at Potsdam with the corresponding times given by Dr. Faris for a group of stations on the western hemisphere, has found almost perfect simultaneity.

I think, then, that the present position of the question cannot be expressed in a better way than by the following statement taken from Prof. Birkeland's work:—

"We may conclude from this that the serrations appear simultaneously, or rather, the differences in time is less than the amount that can be detected by these recordings."

L. VEGARD.

University of Christiania, January 14.

Sir F. Galton and Composite Photography.

MAY I be permitted, as an intimate friend of many years and under deep obligations to the late Sir Francis Galton, to say a word upon a matter which is perhaps not sufficiently emphasised? I refer to his very deep and lasting interest in composite photography, and his conviction of its scientific value. He considered it capable of and well worth systematic development. This was a frequent subject of conversation between us; and he told me many times (sometimes with reference to the original contributions to photography of my brother, Colonel Stuart-Wortley) that he felt the method ought to be developed, not as a newspaper curiosity, but as a serious aid to sociology, and especially to the study of heredity.

Prof. Bowditch, of Harvard, told me that he found

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an unaccountable indifference on the subject in America, while he entirely shared Galton's view of its possibilities.

If anyone could be found to take up the matter seriously there can be no doubt that the pioneer would be richly rewarded. In our last talk, a few weeks before his death, Sir Francis himself told me of really sensational results from the few experiments he was able to make with a comparatively primitive instrument. For instance, he told me he had collected photographs of Queen Victoria and Prince Albert and all their children. To his great surprise, the composite gave the likeness of Princess Alice and no one else. But this was only one of many equally suggestive results.

VICTORIA WELBY.

Duneaves, Harrow, February 3.

Darwin and the Transmission of Acquired Characters.

It is difficult to understand how anyone well acquainted with Darwin's works can come to any other conclusion than that he firmly believed in Lamarck's principle of the transmission of characters acquired by use.

Two clear examples may be cited from "The Descent of Man" (second edition):—

(1) "As the voice was used more and more the vocal organs would have been strengthened and perfected through the principle of the *inherited effect of use*" (p. 87).

(2) "There is no more improbability in the continued use of the mental and vocal organs leading to inherited changes in their structure and function, than in the case of handwriting, which depends partly on the form of the hand and partly on the disposition of the mind; and *handwriting is certainly inherited*" (p. 88).

In this matter Darwin was a true disciple of the great French naturalist to whom Prof. Judd refers with such scant respect.

E. A. PARKYN.

January 30.

I REGRET that your correspondent should imagine that, in writing the words "poor old Lamarck," I showed "scant respect" for the great French naturalist. On the contrary, I desired to express the deep sympathy I felt for this grand pioneer in evolution, who, in old age and blindness, found his splendid achievements, for the time being, discredited by the work and arguments of his successful rival, Cuvier. In the little book which has given rise to this correspondence, I have insisted upon the splendid contributions of Lamarck, not only to botany and zoology, but also to geology, and have shown how the hostility towards his work, felt at first by Lyell and Darwin, was in the end modified, and his great merits acknowledged by both of them.

I quite agree with your correspondent that the passages he quotes—and many similar ones may be cited—show that Darwin accepted the Lamarckian views as to the transmission of acquired characters to a certain extent. Darwin's tendency was, however, to insist that individual variations were always "slight" or "exceedingly little," to use his own words. In the passage to which reference has been made in the "Origin of Species," it would almost seem that he suggests that "variation" had been used in two different senses by authors—variations that could be transmitted and variations that could not be transmitted—and that he demurs to the distinction. I agree with Prof. Meldola, however, in thinking that, in all probability, the view put forward by Prof. Weismann in 1885, that no acquired character is directly inherited, never fairly came under Darwin's consideration.

In discussing questions of this kind, it is important to realise, so far as is possible, what was the current opinion at the time Darwin wrote. Now Baron Cuvier, his brother Frederick, and their followers—whose writings so greatly influenced naturalists in the early years of the nineteenth century—all freely admitted the transmission, by inheritance, of acquired characters, habits, and instincts in domestic animals like dogs; what they denied was that any of the variations so transmitted, so far as the experience of 2000 years showed, were of a fundamental character.

That Darwin not only accepted the idea of the transmission of acquired characters, but even speculated on

the mechanism by which it might be accomplished, is shown by "is invention of the "provisional hypothesis" of pangenesis, has been justly pointed out by Sir William Thiselton-Dyer. In introducing this hypothesis Darwin wrote:—

"A multitude of newly acquired characters, whether injurious or beneficial, whether of the lowest or highest vital importance, are often faithfully transmitted . . . and we may on the whole conclude that inheritance is the rule, and non-inheritance the anomaly" ("Variation of Plants and Animals," popular edition, p. 454)

No mistake can be greater, as it appears to me, than one prevalent at the present day—namely, that by the newer developments of evolutionary theory in Weismannism, Mendelism, &c., Darwin's results are in any way superseded. On the contrary, I firmly believe that had Charles Darwin lived, no one would have more gladly welcomed these new developments than would he; for he would have rejoiced to follow the investigations of the particular methods by which variations are transmitted, the possible limits of individual variation, and the laws which govern their appearance.

Kew, February 1.

JOHN W. JUDD.

Glacial Erosion.

THE reviewer of "Geographical Essays," by Prof. W. M. Davis, writes in NATURE of January 19:—"Prof. Bonney's presidential address to the British Association has brought the controversy on glacial erosion to a head. It may be hoped that the authoritative and masterly statements on both sides will lead to an agreement as to the main facts, but no settlement can be expected until the arguments of those who limit the efficacy of glaciers as eroding agents have been directly answered."

I do not think that those who, like myself, hold that glaciers are powerful eroding agents would shrink for a moment from directly answering their opponents' criticisms. The most direct answer is that the deposits formed by glaciers are a direct measure of glacial erosion. I distrust all theoretical opinions based upon the study of ice as a "rock." In the early days of geological science it was difficult to convince the many that the "purling brook" and the "babbling river" had frequently excavated the deep valleys and gorges through which they run.

Do the opponents of glacial erosion really contend that the enormous deposits of boulder clay which cover such extensive portions of England, Scotland, and Ireland are not the results of glacial erosion? I say boulder clay advisedly; for there are immense deposits of laminated clay with or without boulders, sands, and gravels, which some may argue have no connection with glaciation. Here, however, I should again differ, for many years of careful study in the field have convinced me that nearly all these superficial or "drift" deposits are the result of glacial erosion.

Taking the "glacial" deposits themselves as a measure of glacial erosion, and concluding that we must look for marked effects in the areas from which the material was eroded, what do we find? We find surface lowland features, valley gradients, valley forms, and entire valleys and gorges, which are not such as are produced by the erosive action of water, rain, and frost.

The opponents of glacial erosion have been too much guided by glacial action, as now seen in such mountainous areas as Switzerland. The puny glaciers now found there cannot be compared, so far as the effects they produce are concerned, with the great confluent glaciers which once occupied the valleys.

It is a pity that in this country the conviction which so many hold concerning glacial erosion and climatal changes should have resulted in the stagnation of glacial geology as a science, for it cannot be denied that if glaciers have done very little as agents of change, there must be very little to study.

Glaciologists of the active school cannot but feel grateful to such workers as Prof. James Geikie, Prof. W. M. Davies, Prof. R. S. Tarr and others, for keeping the lamp burning.

Inglewood, Longcroft Avenue, Harpenden,
January 18.

R. M. DEELEY.

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HARDLY anyone disputes that the passage of ice over the British Uplands swept away all the loose rock materials and redeposited them in the Lowlands as glacial drifts. The controversy is not as to the removal of the loose débris, but of the excavation of basins in fresh hard rocks. As Mr. Deeley states, the opponents of glacial erosion have written extensively; but certain serious difficulties that have been advanced by Prof. Bonney, Prof. Garwood, and others, do not seem to me to have been directly answered. I share Mr. Deeley's gratitude to the three geologists whom he names for their important contributions to glacial geology.

J. W. G.

An Unconscious Forecast by Joule

THE following remarks by Joule in his paper on the changes in temperature produced by the rarefaction and condensation of air (*Phil. Mag.*, May, 1845) are worthy of notice:—

"The beautiful idea of Davy, that the heat of elastic fluids depends partly upon a motion of particles round their axes, has not, I think, hitherto received the attention it deserves. I believe that most phenomena may be explained by adapting it to the great electrochemical discovery of Faraday by which we know that each atomic element is associated with the same absolute quantity of electricity. Let us suppose that these atmospheres of electricity, endowed to a certain extent with the ordinary properties of matter, revolve with great velocity round their respective atoms. . . ."

"The phenomena described in this paper, as well as most of the facts of thermochemistry, agree with this theory; and in order to apply it to radiation we have only to admit that the revolving atmospheres of electricity possess, in a greater or less degree, according to circumstances, the power of exciting isochronal undulations in the ether which is supposed to pervade space."

In the idea of the "atmosphere of electricity" revolving round the atom, we have the substance of J. J. Thomson's corpuscular theory, while the electromagnetic mass of the revolving "atmospheres of electricity" would certainly cause them to be "endowed to a certain extent with the ordinary properties of matter." Again, the last phrase of the extract is simply the modern idea of electromagnetic waves in the aether.

The premature birth, in this short quotation, of three of the most startling advances of modern physics is not a little remarkable.

B. A. KEEN.

University College, London, January 25.

The Sailing-Flight of Birds.

IN a letter to NATURE in February, 1876, I suggested that the sailing-flight of birds and the flight of flying-fishes could be explained as tobogganing under almost perfect conditions, and in 1889 the late Duke of Argyll accepted this, in a letter to the *Spectator*, as a correct and sufficient explanation. My old friend the late Prof. H. N. Moseley, a member of the *Challenger* staff, held the view that a quivering, imperceptible to the eye, of the wings and fins was the true explanation. I do not know which explanation has been generally accepted, but I would suggest that a kinematographic picture of the flying-fish ought to settle the question finally, if it is not already settled.

I said in my letter:—"By means of a suitable mechanism for changing the inclination of the wing-planes every few seconds the sailing-flight of the albatross, I believe, might be simulated without much difficulty." Has not the aéroplane done this?

R. ABBAY.

Earl Soham Rectory, February 1.

A Morning Meteor.

A METEOR equal in brightness to the Pole Star, and of much the same colour, was seen by me to fall from the southern sky at 6.25 on the morning of Friday, February 3. Its path was one of ten degrees, extended along a line midway between a Corona Borealis and the planet Jupiter, which at that time was shining lustrously some thirty-four degrees south, and slightly east, of Arcturus. The meteor left a steel-blue train which remained visible for six seconds.

JOSEPH H. ELGIE.

72 Grange Avenue, Leeds.